

a formulation of the principle upon which 'the American school' acts in applying this method of nomenclature. The following paragraph is taken from a plate-proof of my new 'Key,' p. 76, long since stereotyped, but not yet published:—

"No infallible rule can be laid down for determining what shall be held to be a species, what a conspecies, subspecies, or variety. It is a matter of tact and experience, like the appreciation of the value of any other group in zoölogy. There is, however, a convention upon the subject, which the present workers in ornithology in this country find available; at any rate, we have no better rule to go by. We treat as 'specific' any form, however little different from the next, that we do not know or believe to intergrade with that next one; between which and the next one no intermediate equivocal specimens are forthcoming, and none, consequently, are supposed to exist. This is to imply that the differentiation is accomplished, the links are lost, and the characters actually become 'specific.' We treat as 'varietal' of each other any forms, however different in their extreme manifestation, which we know to intergrade, having the intermediate specimens before us, or which we believe with any good reason do intergrade. If the links still exist, the differentiation is still incomplete, and the characters are not specific, but only varietal, in the literal sense of these terms. In the latter case, the oldest name is retained as the specific one, and to it is appended the varietal designation: as, *Turdus migratorius propinquus*."

While it is always safer to prophesy after than before the event, I nevertheless venture to predict that the nomenclature of the near future will fully recognize some such principle as this, and apply it by means of trinomial nomenclature, in Europe as well as in America, and especially in Great Britain. In my judgment, the interests of the B. O. U. and of the A. O. U. would both be subserved by an alliance in this particular.

Very truly yours,

Washington, February 20. 1884.

ELLIOTT COUES.

Are Trinomials Necessary?

TO THE EDITORS OF THE AUK:—

Sirs: I feel sure that every amateur who has read the reply to my letter in the January number of this magazine will feel as sincerely thankful for it as I certainly do—grateful for the information conveyed, and pleased to have the proof that such questions as I have asked will receive kind and courteous consideration in the pages of 'The Auk.'

Candor compels me to add, however, that the reply has not, in some points at least, proved entirely convincing, and I return to the subject for the purpose of gaining further light.

It is to be hoped that the more advanced students will not grow impatient over the persistency and, perhaps to them, apparent stupidity of these unbelievers of the 'amateur element.' Those who have passed from unbelief to a firm conviction that trinomials are useful and neces-

sary should remember that they gained such happy consummation only through a gradual process of reasoning, and should not expect those to whom the subject is comparatively new to reach the same plane of thought at a single bound. For every man, worthy the name of student, will ask a reason for each successive step, and not take them simply at some other man's *dictum* — the day for that has passed.

That which is very generally considered antagonism to trinomialism is not, so far as the American amateur ornithologists are concerned, an opposition to the use of three terms to distinguish varieties from species, but an unbelief in the necessity of recognizing varieties by *any* distinctive appellation. Prove to us that varieties are a necessity, that trinomials are an advance toward 'exactness of expression,' without an overbalancing loss in complication and increased difficulty in study, and we will accept the trinomial pure and simple — without any connecting term — as an improvement upon any previous method of denoting these forms. We harbor no 'Dr. Dry-as-dust' 'craze' for a purely binomial nomenclature, but we do protest against the propagation of any system which unnecessarily creates obstacles to the study of the science, instead of simplifying it; we do ask that our leaders shall not take a step backward and force upon us something which is barely more than a change, and not only no improvement, but a palpable injury; that we be not dragged into a 'craze' for trinomialism by following the lead of an 'American school,' in whose splendid abilities and brilliant performances every American amateur feels a glowing pride.

But while stating all this I must not allow it to be thought that the unbelievers are blind to the possibility of their unbelief being based upon misunderstanding, or perhaps ignorance: they fully realize that this may be the case — hence these questions.

Our stumbling-blocks may be stated in a few words. We conceive that the recognition of varieties tends to create confusion in classification and nomenclature, and increases the difficulty of identifying specimens. We do not see that by it any advance toward exactness has been secured; and it appears to us that to gain this advantage, and to be consistent, and carry to its legitimate end the argument for their adoption, *every* variation from a given type must receive a distinctive name; necessitating not alone the recognition of varieties of species, but also of varieties of varieties almost without limit.

This idea is, of course, too absurd to deserve a moment's earnest thought; and, considering that zoölogical classification is to some degree artificial, and that only an approximation to complete exactness can be reached, we are forced to the conclusion that, on the whole, a better result would be accomplished if those forms which are sufficiently differentiated to demand a distinctive name were classed as species instead of varieties — that when a Song Sparrow ceases to be a Song Sparrow it were called something else — and a pure and simple binomial nomenclature were thus retained, the less important variations being understood through the medium of a general law.

One point more. I have seen it stated (I cannot at this moment remember just where), that the fact that all recent American writers on ornithology use trinomials is evidence that they endorse the system which these trinomials represent. Now, as a matter of fact, this is not true; but even if it were true, the statement could not be made fairly upon such evidence. It is well known that the prevailing systems of nomenclature have been adopted by the large majority of American writers simply because they *were* the prevailing systems, for convenience's sake. And if some bolder and more independent spirit were to rebel against following a prevailing idea with which he did not agree, and attempt to originate a system for himself, he would very probably be 'set upon' as a conceited, self-opinionated person, and have hurled at his heretical head some such crushing sarcasm as 'he has become almost an ornithologist.'

The 'amateur element' appreciate the desirability of having all classes of American ornithological students work in harmony and in concert, and it is for this reason that the unbelievers in trinomials desire to get rid of their unbelief.

Respectfully yours,

St. John, N. B.

MONTAGUE CHAMBERLAIN.

[We are glad to see that the objection to trinomials is not, as we in writing our former reply supposed, that they are in themselves an objectionable innovation in nomenclature—as opposed to strict adherence to binomialism—but that their acceptance depends upon proof that it is necessary to recognize varieties, or incipient species, at all in nomenclature. We return to the subject, however, with a feeling that the doubters may not be open to conviction by such evidence as can be readily put before them on paper; but that their conversion would be easy could we lay before them series of specimens illustrating the forms to which trinomials are applied, showing them how different many of them are in their extreme phases of divergence, and at the same time how completely they inosculate.

As stated in our former reply, the best, and in fact most, naturalists the world over believe it necessary to, and in practice do, recognize varieties as a means of giving a correct and precise expression to the status and relationship of a grade of forms differentiated to a degree that renders their recognition in nomenclature necessary if we would properly formulate the facts of biology, although such forms are known to intergrade and cannot, therefore, properly rank as species. Furthermore, the recognition of varieties is much more prevalent now than formerly, in consequence of better knowledge of the relationships and real status of such forms, resulting from more favorable opportunities for study and the rapid accumulation of material. Although Mr. Chamberlain does not even imply that the 'unbelievers' of the 'amateur element' think they have a better knowledge of what is required in the case than the specialists—the experts in the subject, who are not only trained naturalists, but who have had in hand an amount of material, and opportunities for judgment in such

questions, of which some at least of the 'unbelievers' have little conception—yet we can hardly believe it unkind on our part to ask the 'unbelievers' to answer for themselves the questions, whether expert testimony, in matters of science at large, or in human affairs in general, is entitled to any more weight than lay opinion; or whether if they had had the same opportunities for study, and the same amount of material for investigation, they believe that they would have reached other than the same conclusions, or would have taken any different course of action.

As to varieties and trimonials making nomenclature more complicated, and the study of ornithology more difficult, is not the difficulty complained of necessarily inherent in the subject, and dependent rather on the degree of knowledge the student aspires to acquire, than on any needless encumbrances thrown in the way by the 'leaders' in the science?

But our correspondent will, we fear, think, in this instance at least, that our reply is not only ungracious, but that we are seeking to evade the issue he presents. We must therefore say, that to discuss the subject in its many bearings, and in a way to present in argument what could be quickly and easily shown by recourse to specimens, would require a long essay rather than the few paragraphs here at command. So we must content ourselves with adding to what was said or implied in the reply to our correspondent's former letter, and in Dr. Coues's letter above given, that a philosophic principle underlies the whole subject, and that it is not merely a matter to be decided by 'convenience.' While classification is to some extent conventional, the object of classification in zoölogy is to express the natural or genetic relationship of the objects classified; and the proper distinction of varieties from species is by no means an unimportant element in this scheme. The 'unbelievers' for whom Mr. Chamberlain speaks are not to be presumed to be so skeptical as to ignore the modern doctrine of evolution; and, viewed from this standpoint, it makes a vast difference whether we indifferently term a given form a 'species' or 'variety' in obedience to a mere principle of convenience. As Dr. Coues above states, the recognition of a form as a species implies "that the differentiation is accomplished, the links are lost, and the characters actually become 'specific.'" By varieties are meant forms that are not fully differentiated — in other words 'incipient species,' or species still in the process of evolution. It hence follows that the terms species and varieties are not interchangeable at will, but expressions for certain definite and known facts in nature, grounded on a philosophic principle, to ignore which is not only unscientific, but is to deprive us of a means of precise definition at a point where precision is of high importance. As we said before, and as Dr. Coues restates, the determination of how great a divergence from the common stock a form must have to render it desirable to recognize it in nomenclature, "is a matter of tact and experience, *like the appreciation of any other group in zoölogy.*"

As is well known, no two individuals of any species are exactly alike; yet it would be absurd and useless, were it not also impossible, to give names to each. There are also many local variations that are not too

slight to be detected, but which are either too slight or too inconstant to require recognition. While theoretically it is possible to recognize 'varieties of varieties,' in practice this rarely occurs, and should never be countenanced; if a form is different enough to be recognized, it should stand as a variety of the common stock, not as a variety of a variety, although it may be more nearly related to some one of several varieties than to any of the others.* Again, the objection has been raised that the recognition of varieties is subject to the caprice of any dabbler who may feel disposed to set them up: theoretically this also is true, but in practice such work falls where it should — to experts, who occasionally err in judgment, or through inadequate material, but in the main are safe guides, and as such are followed, even by their peers when these have not themselves the same or a better opportunity to review the group in question. The recognition of a variety is a matter to be as carefully and conscientiously considered as the recognition of a species, or any higher group.

Hoping that our remarks may serve to throw a little further light upon the points at issue, we again take leave of the subject.—J. A. A.]

The Ornithological Report in the 'Cruise of the Corwin.'

TO THE EDITORS OF THE AUK:—

Sirs: I observe that in his notice of my ornithological paper in the 'Arctic Cruise of the Revenue Steamer Corwin,' Dr. Coues indulges in some severe strictures on the typographical errors and mechanical execution of the report.

It must be conceded that the number of these errors and their atrocity renders his critical remarks justifiable enough. Had, however, Dr. Coues

* In this connection it seems not out of place to refer briefly to a point raised by Dr. Stejneger in his article in this number of 'The Auk' on the genus *Acanthis*. He alludes (p. 150) to Mr. Seebohm's practice of forming trinomials of the names of the conspecies most nearly related, as tending to better express their true affinities than does the method, adopted by American writers, of taking for the second term of the trinomial the name first given to the group of conspecies as a whole or to any of its forms; and adds: "This . . . is a point which merits earnest consideration." We believe, however, that there are two unquestionably strong objections to Mr. Seebohm's method of constructing trinomials. First, it leaves the construction of conspecific names subject to individual opinion as to what two forms of a given group of intergrading forms are most nearly related — a point about which there must, in the nature of the case, be often a diversity of opinion. Second, and of far greater importance, it ignores the law of priority — the fundamental principle of our nomenclature — and therefore opens the way to instability of names and endless confusion. It seems to us perfectly evident that the law of priority should be considered as equally imperative in relation to conspecific — or subspecific — names as to specific and generic names. In other words, the name first applied to any form of a group of conspecies should be the designation, in a specific sense, for the group as a whole, and should also form the second term of the trinomial for each of its conspecies, whatever may be their relationship *inter se*; and that the slight gain accruing in special cases by Mr. Seebohm's method is much more than offset by the ill results that must inevitably follow from disregarding the law of priority in constructing conspecific names.